

THE MODE OF INFECTION
IN MALARIA ;

THREE MORE ROSS-MANSON
LETTERS, 1898.

BY:

SIR RONALD ROSS, K.C.B., K.C.M.G., F.R.S.,
Nobel Laureate.

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THE MODE OF INFECTION IN MALARIA; THREE MORE ROSS-MANSON LETTERS, 1898.

PREFATORY NOTE.

LAST month I published my two letters of August 22nd and 31st, 1897, to Dr. Patrick Manson, in which I recorded the finding of the malaria parasite in the stomach tissue of two *Anopheles* mosquitoes. I now publish my three letters of June 28th, July 6th, and July 9th, 1898, regarding the unexpected discovery by me that the parasites of malaria are communicated by the *bite* of the infected mosquitoes through their salivary glands. There was no guide at all to conduce us into the opinion that the parasites are communicated in such a manner. There was no reason to believe this and no evidence directing our thoughts along that line. The discovery amazed me when I made it, Manson when he heard of it, and all those who read of it; and we are still astonished at the fertility of invention possessed by nature and derived merely from her trial-and-failure methods. Obviously, even if the infection does succeed in being carried by this method, it will succeed only in a minute proportion of the parasites present in the mosquito at any one time.

As a matter of fact, this portion of my work was practically the most important portion, because it showed us exactly how the *contagion vivum* is carried and how it could be avoided. If I had not succeeded in showing the exact route, one can imagine the immense amount of talk that would still have been to follow, even after the mode of entry of the parasite into mosquitoes was demonstrated. Unfortunately, though this subject has been before medical men for more than 30 years, most of them seem still to be under the delusion that this part of the discovery was one of trifling and of secondary interest—showing how long it takes them to *understand* even demonstrated facts. It is of little importance that a mosquito should carry the poison *out* of us, but one of *vast* importance that

it should also put the poison back into us ; *and this was no part of Manson's original teaching*. Strangely enough, some writers do not even trouble to mention the facts ; yet, as I have said, there was nothing in previous theory to guide us towards this portion of my work, which was done with infinite labour months before anyone else had even found the malaria-parasites in mosquitoes at all.

I must again explain that, as I was working alone in 1898 except for the letters from Manson, which *from time to time* gave me useful advice, I was forced to invent the whole of the necessary *technique* for myself ; also that, as I could obtain little information about mosquitoes in India at that time, my letters contained very little matter of purely entomological interest, except that they may live for weeks.

The observations recorded in the previous letters were made with human malaria. They discovered two vital links in the chain—and the two unknown quantities most difficult to discover—namely, the kind of mosquito which carries the parasite, and the exact situation and appearance of the malaria-parasite in the mosquito.

In the three letters now published I was working with a parasite of birds, and not with a parasite of men, as owing to many difficulties I was not able to deal with the latter. In the birds, I soon confirmed the work previously done with the malaria of man, and in due course worked out the full life-history of the parasites in birds. Having proved that the zygote stage was identical in the stomach of birds and in human malaria, there was every reason to believe that the later stages of human malaria would also be identical, as in fact subsequently transpired. Nothing essentially and fundamentally new was, therefore, supplied by the subsequent investigations on human malaria.

My thanks are due to Miss Maude Lafford for the trouble she has taken over the proofs of this book.

RONALD ROSS,

Ross Institute and Hospital for Tropical Diseases,
Putney Heath, S.W.15.

December 31st, 1929.

ERRATA.

Page 2, line 19, *for* “*contagion*” *read* “*contagium.*”

„ 4, „ 24, *delete* “the stomach of”.

„ 9, „ 16, *after* “this” *read* (*sic*).

„ 9, „ 33, *for* “Rs. 10,000” *read* “Rs. 1,000”.

„ 11, „ 16, *after* “of” *read* (*sic*).

„ 16, „ 11, *for* “fluid” *read* “field”.

COPY.

CALCUTTA,

June 28th, 1898.

DEAR DR. MANSON,

I despatched some copies of my report to you by this mail with one for Sir J. Fayrer. Will you oblige me greatly by sending it on to him. I don't know his address, but will write to him as well.

I fear much I shan't be able to send you good second-day specimens as my infected birds are so much better that they don't yield good crops any longer ; but will try to-morrow.

The bulk of the copies of my report are not sewn even yet and will never reach the Secretary of State through the Government of India in time for the B.M.J. Meeting. The only thing to be done then is for you to send him one of the copies I have despatched to you with Leslie's letter, and ask his permission to publish. He is sure to give it straight off. Another advance has been made. On the 6th and 7th day the coccidia form a large number of germinal rods (?) 12μ in length. This is easily seen by using a rather strong salt solution which gives many of the coccidia from 5th day a striated appearance as figured in the Report, but more marked. By using pressure these coccidia are easily burst, when the rods pour out.

They are just about 12μ in length and all the same size. They taper toward each end and are therefore thicker in the middle. In salt solution after a time they show one or more vacuoles in the middle part and become bent angularly. They stain easily with Loeffler, Delafield, etc., and then show one or more chromatin granules toward the middle, between the vacuoles, and at the same time become broader, when they look very like small *trypanosomes*. They often possess swellings somewhere along their length, very like flagella do. On addition of water they tend to curl up and look like granular material. I am not sure whether they are at all motile though I have often thought so. I think especially that they bend and straighten themselves but these points are very difficult to ascertain because of their continual brownian movement, while it is evident that both water and salt solution kills them. (See photo I.)

When youngish striated coccidia are burst these bodies issue

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tinual brownian movement
while it is evident that br. &
water & salt solution kills
them.

Photo I.*

attached like rays to circular cells, and others similar. (See photo II.)

This arrangement can often be seen within the unburst coccidium.



When youngish striated coc-
cidia are burst these bodies
issue attached like rays to
circular cells.

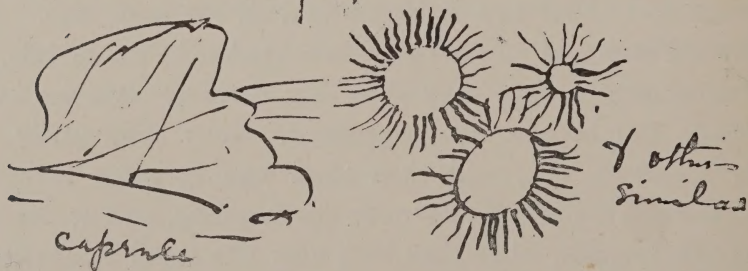


Photo II.*

The circular cells to which the germinal rods are attached by their thin end stain easily like the rods, but show no sign of nucleus.

In older coccidia the rods pour out all separately and the cells are not to be found.

The rods are easily seen in large numbers in eight-day mosquitoes (and earlier) all round the stomach and amongst the abdominal scales. They are in the caelom.

The use of fairly strong salt solution has enabled me to ascertain these points, which may be taken as quite certain.

No development of the rods has yet been observed either in living or in decomposing mosquitoes.

One never finds all the coccidia, even when mature, to be striated—that is, to contain rods. The rest seem to contain nothing but a clear fluid with bright oil globules.

I have searched in vain in living mosquitoes and dead ones for anything like psorosperms or pseudo navicellae, unless the black sausage-shaped bodies (fig. 20 in the Plate of my Report)¹ are such. These however are only rarely seen and appear then to exist only in empty capsules. I expect they are *fungi* or something—germs within germs but am keeping my eye on them.

It appears pretty certain that on the 7th or 8th days all the coccidia burst in the *living* insect and pour their contents into the caelom.

Are the rods of the nature of *spermatozoa*, or are they *falciform* bodies? In the latter case the round cell with its radiation of rods (see photo III) may perhaps be the homologue of the ordinary spore of coccidia—in fact a *naked spore*.

2 into the 'caelom'.
 Are the rods of the na-
 ture of spermatozoa, or are they
 falciform bodies. In the caelom
 the round cell with its
 rods may perhaps
 be the homologue of the rods
 spores of the coccidia—
 a naked spore.

Photo III.*

¹ Report on the cultivation of *Proteosoma*, Labbé, in Grey Mosquitoes, 'Ind. Med. Gaz., 1898,' XXXIII, No. 12, December.

* Photographs of the original letter of June 28th, 1898, in the possession of the London School of Hygiene and Tropical Medicine.

Nature probably makes some extraordinary effort here in order to complete the life cycle. What the dickens she is going to do next I cannot imagine at all. Great difficulties may however be feared owing to the absence of hard spores. But I am trying to turn the position by blind attempts to infect healthy sparrows.

Infected mosquitoes have been fed on five healthy sparrows for a fortnight. On examining again the other day a few proteosoma were found in one of the sparrows—scarcely good enough. I am therefore feeding three healthy sparrows with mosquitoes containing mature coccidia.

But this climate! I can perhaps stand another month of it. It is hell, simply; and the constant strain on mind and eye at this temperature is making me thoroughly ill. If I cannot infect birds or find a clear opening by the end of July I *must* clear out. Permanent work here is out of the question for me, I feel. But I shall stick on to the last gasp in order to be able to wire to you before the B.M.J. meeting, if possible.

I have heard from Laveran. He has received my specimens and accepts the discovery. He writes: "J'ai examiné tous vos préparations et j'ai constaté que vos descriptions étaient d'une exactitude parfaite. . . . il ne semble pas douteuse qu'ils (the coccidia) dérivent des hématozoaires des oiseaux. . . . Je pense comme M. Manson que vous avez fait une découverte très importante." Perhaps, however it was not very wise to put the thing in his hands at all.

June 29th. I have found large numbers of germinal rods in the thorax of two mosquitoes, one a *six*-day and one an *eight*-day one. The former contained two empty capsules and two striated (rod-bearing) coccidia; the latter contained only a large number of empty capsules. The rods lay among the scales and muscles of the thorax and showed no further development.

This scarcely looks like the spermatozoa theory. The rods appear much too numerous for this and sometimes *all* the coccidia in a mosquito are rod-containing ones. They must evidently get into the insect's circulating fluid for some reason.

Now I must thank you for yours of June 6th. This reached me, being addressed to c/o Postmaster. But the last one before this

was dated April 29th, so that I don't know how the specimens arrived. I judge that they were not very good. It must be due to the voyage as my own remain all right for months. I sent you all my best specimens last mail (except one series which I keep for demonstrations), and I hope that these, with a copy of my report will do for Lord Lister. The *best* specimens are sometimes those with the insect's scales in them, put in for the purpose of preventing the cover glass from crushing the larger coccidia.

Thin's advice to work for years and years is good. It is just what I have done. It is also good not to publish until there is not a vestige of objection possible. Here again the Report follows his advice.

My advice is that any further objections are scarcely to be recommended, as they will meet with a terrible fate, I think. I don't see any line of criticism which can be taken up against the theory with any chance of success. I venture to say that this is no fact in the life history of any parasite more certain than that proteosoma in the bird becomes coccidium in the mosquito. It is quite certain. The man who criticises will die; he will knock his head against a rock.

Many thanks for your efforts on behalf of a commission. Remember always two things that the Government of India is a *mule* as regards science. It won't do anything unless driven. As for the I.M.S., it does not care a cent for the whole business. Remember also that if the Government of India won't do anything I am always willing (if not anxious) to take my pension and work elsewhere and for other masters. What do you think of Egypt? Fact is we are tired of India and I feel that my health won't stand it very much longer. Besides research work will never pay me in cash out here, and it will not lead to anything that will. It simply means heat and bad health—unless Government will put me some height up the hills, which I doubt. Monetarily my pension of £300 a year is really almost equal to my present pay of Rs. 10,000 a month, with the expenses of life in India with continual moves. So if there is anything going elsewhere which is open to me and is better than India, remember I will take my pension at once to get it.

Several specimens of anchylostoma eggs, trypanosomes and filariae of birds which I put up for you to have, I just find, have been accidentally left at Kurseong.

Wishing you every luck with the Tropical Section,

I remain, yours sincerely,

(Signed) R. ROSS.

Leave orders to forward letters and telegrams for you to Edinburgh, as the copies of the Report¹ may come late and I may be able to wire something to you at the last moment.

COPY.

CALCUTTA,

July 6th, 1898.

DEAR DR. MANSON,

Let me in the first place felicitate you and the profession on the new Tropical Section and let me wish it every success in the future. It is started under the best possible auspices.

I hope this letter will reach you at Edinburgh. If so, it will be opportune; because I feel *almost* justified in saying that I have completed the life-cycle or rather perhaps one life-cycle of proteosoma, and therefore in all probability of the malaria parasite. I say almost, because though I think I have seized the final position, I have not yet occupied it with my full forces. My last letter left me face to face with the astonishing fact that the germinal rods were to be found in the thorax as well as in the abdomen. Instead of the hard resisting spores we expected to arrive at—spores easily seen and followed—here were a multitude of delicate little threads, scarcely more visible than dead flagella and poured out amongst the million objects which, under an oil-immersion lens, go to make up a mosquito. I daresay you imagined my consternation. I could not conceive what was to happen to the rods.

Well, I was in for a battle. It was, I think, the last stand—on the very breathless heights of science. I am nearly blind and dead with exhaustion! but triumphant. Expect one of the most wonderful things.

¹ Report on the cultivation of *Proteosoma*, Labbé, in Grey Mosquitoes, 'Ind. Med. Gaz.', XXXIII, No. 12, December.

The rods were evidently in the insect's *blood*. By merely pricking the back of the thorax and letting the milky juice flow into a minute drop of salt solution thousands of *proteosoma-coccidium* rods could easily be obtained. The question was what next?

I now divided my insects before dissection between the thorax and abdomen and examined each part separately. It was found that the rods were often *more numerous in the thorax than in the abdomen*. There were even cases where scarcely one could be found in the abdomen (the coccidia having evidently burst some days previously as shown by their empty capsules), while numbers (4 or 5 in a field) could be detected on teasing up the thorax and head.

Here however I was brought up standing. Sometimes the rods were more common in the head, sometimes in the thorax. I went at mosquito after mosquito, spending hours over each, until I was blind and half-silly with fatigue. The object was to find if possible a place of structure where the rods accumulate; or to discover some further development in them. Nothing.

On the 4th, however, after pulling out the head by its roots (*aesophagus*, etc.) from the thorax, some delicate structure dropped out of the cervical aperture of the latter. This proved to be a long branching gland of some sort, looking like a cell of large intestine, and consisting of a long duct with closely packed refractive cells attached to it.

I noticed at once that the rods were swarming here and were even *pouring out* from somewhere in streams. Suddenly to my amazement, it was seen that many of the cells of this gland *contained the germinal rods of proteosoma-coccidia within them*. Looking further, the cells of one whole lobe of the gland were simply packed with them, and on bursting the cells the rods poured out of them just as they pour out of the original coccidia. (See photo IV.)

The rods were quite unmistakable, having the tapering, flattened and vacuolated structure peculiar to them. They are identified at once and no structures like them exist in the normal mosquito. Here they were in the cells of the gland. The cells were not coccidia. They are only 25μ in diameter, have a very thin outline and contain a perfectly clear fluid, without granulations or oil-drops such as the

coccidia possess. The rods lay within them quite irregularly and motionless, except for brownian movement. In one lobe almost every cell contained numbers of rods; in other lobes only one or two cells contained them. By the attachment of the cells to the central duct, it seemed quite easy for the rods to pass on occasion from the former into the latter.

Looking further the cells of one whole lobe of the gland were simply packed with them, and on bursting the cells the rods poured out of them just as they pour out of the original coccidia.

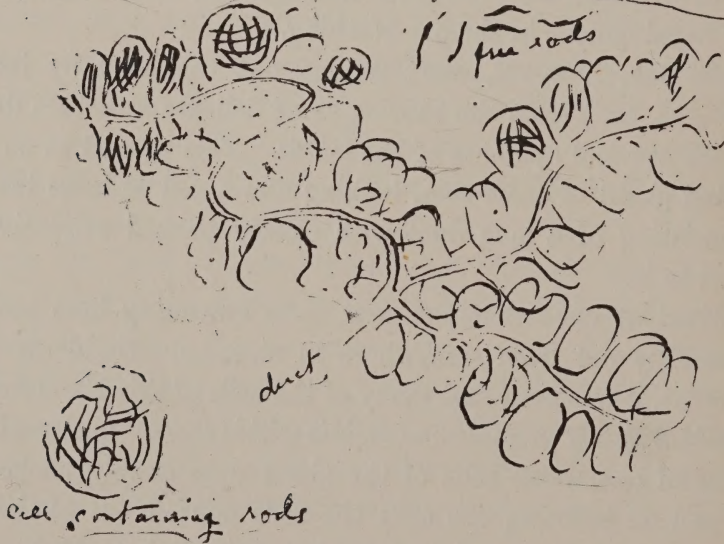


Photo IV.*

Now what was this gland? Will you believe it, I examined two whole mosquitoes without finding it again? What with the scales, the débris of muscles, etc., I could not come upon it. A third mosquito gave the same result, until I opened the head itself. There was the gland attached by the duct which led straight into the structures

somewhere between the eyes. The cells were again packed with germinal rods. I have found the gland now altogether in seven mosquitoes. In six of them the cells were packed (especially in some lobes) more or less with germinal rods. In the seventh I could find only a small piece of gland, which was free from rods.

I still experience, however, the greatest difficulty in dissecting out the gland itself. It appears to lie in front of the thorax close to the head, but breaks so easily in the dissection that I cannot locate it properly. In the second mosquito however there was no doubt, as shown by evident attachment, that the duct led straight into the head-piece, probably into the mouth.

In other words it is a thousand to one, it is a *salivary gland*.

I think that this, after further elaboration, will close at least one cycle of proteosoma, and I feel that I am *almost* entitled to lay down the law by direct observation and tracking the parasite step by step.

Malaria is conveyed from a diseased person or bird to a healthy one by the proper species of mosquito, and is inoculated by its bite.

Remember however that there is virtue in the "almost." I don't announce the law yet. Even when the microscope has done its utmost, healthy birds must be infected with all due precautions.

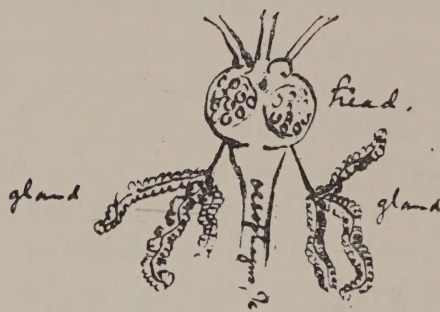
I say *one* cycle. I think it likely there is another. I continually observe that only a portion of the coccidia contain germinal rods. The rest, I now think, give rise to the black sausage-shaped bodies shown in fig. 20 in the Plate of my Report,¹ which I believe may be the true spores of the parasite meant either for free life or to infect grubs. Oddly enough, in old mosquitoes these bodies also get carried away into the tissues—unless they are some disease of the insect. I will attack this next.

7th. I dissected two healthy mosquitoes this morning and began by dividing the head and anterior third of the thorax from the middle third by means of a razor, and then carefully breaking up the anterior third. In both cases the glands were found and their ducts were traced straight into the head (see photo V).

¹ Report on the cultivation of *Proteosoma*, Labbé, in Grey Mosquitoes, 'Ind. Med. Gaz., 1898,' XXXIII, No. 12, December.

In all probability it is these glands which secrete the stinging fluid which the mosquito injects into the bite. The germinal rods lying, as they do, in the secreting cells of the gland, pass into the duct when these cells begin to perform their function, and are thus poured out in vast numbers under the skin of the man or bird. Arrived there, numbers of them are probably instantly swept

head ; thus : —



In all probability it is 'thus' which secretes the stinging fluid which the mosquito injects into its

Photo V.*

away by the circulation of the blood, in which they immediately begin to develop into malaria parasites, thus completing the cycle.

No time to write more.

Yours very sincerely,
(Signed) R. ROSS.

COPY.

CALCUTTA,
July 9th, 1898.

DEAR DR. MANSON,

One full solution of the malaria problem was obtained this morning—unfortunately just too late to catch the mail by wire to Bombay. The facts are as follow :—

Just after I wrote to you I obtained some good dissections of the gland in healthy insects. It still remains very difficult to get out

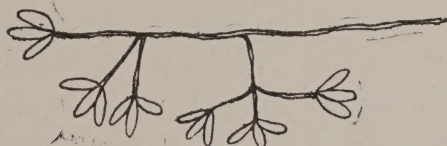
* Photographs of original letter of July 6th, 1898, in the possession of the London School of Hygiene and Tropical Medicine.

nicely and almost impossible to mount for you; but by examining a number of insects I have obtained the leading facts about it. The lobes lie either in the head, neck or front of thorax (according to the fulness of the cells, I think). I think there are 18 lobes (see photo VI).

The ducts finally all join together into one large tube which runs up along the under surface of the head. In one specimen which I will send you I can trace it to its termination. When it arrives close to the origin of the mouth parts, it (the duct) passes through a kind of round shield and enters one of the long stabbing spears of the proboscis. It appears to run up *inside* this and open somewhere near its end.

ording to the fulness of the cells, I think

18 lobes: - thus



ducts finally all join together into one lar

ong the under surface of the head. In one

Photo VI.*

There is no doubt, then, that the gland must pour its secretion straight into the wound made by the mosquito.

In old infected mosquitoes I think that many lobes of the glands are almost destroyed by infection with the proteosoma rods. In one case the cells seem to have united into a single sac loaded with rods.

In 8 male mosquitoes I have not once found any evidence of the existence of this gland.

So far, then, there is a complete chain of evidence pointing to the infection during haustellation. Now for the clincher.

On June 25th (as I think I told you) I carefully selected three healthy sparrows. Their blood had been examined three times on different occasions and always found free from parasites. On the

* Photograph of typewritten copy of original letter of July 9th, 1898, in the possession of Sir Ronald Ross,

24th night and almost every night following I have used these birds to refeed a large stock of mosquitoes which had been infected from diseased sparrows on June 21st-22nd. This was the identical stock with which I worked out the story of the germinal rods. Lately I had been finding young crops of coccidia in this stock and therefore had a presentiment of what I should observe in the birds. I wish therefore I had examined them before, so as to let you know in time for the B.M.A. Meeting; but I hardly expected such a knock-down result.

All three birds, perfectly healthy a fortnight ago, were now simply swarming with proteosoma, 20 parasites and more in one oil-immersion fluid.

Again on July 1st I selected a healthy sparrow and a healthy "baia," and refeed another infected stock on them every night up to date. These two were examined this morning. Both have proteosoma: the sparrow a few, the "baia" fairly numerous.

Hence I think I may now say Q.E.D., and congratulate you on the mosquito theory indeed. The door is unlocked, and I am walking in and collecting the treasures.

For instance, proteosoma of sparrows is communicable to "baias"; hence, there was probably only one proteosoma, common to various birds. You see, I shall give it to crows, etc. Also, I have an idea indeed. What if proteosoma = spring tertian? I shall feed infected mosquitoes on a man or two, and shall take it from a man to a sparrow, if it be so.

July 11th.—I have forgotten to say that 10 healthy sparrows examined on June 25th and kept nightly in mosquito nets to keep mosquitoes *out*, all remain perfectly well up to date. I told you also that on June 25th I gave infected mosquitoes to three healthy sparrows by the mouth. Two of the birds died a few days afterwards (numbers of my birds have been dying) without proteosoma. The third had nothing on July 1st and was used for feeding infected sparrow, with positive results as already given. Rather a jumble, but future experiments will be more precise. It's useful to be jumbly at times to begin with.

I have now to record three more successes. Three "baias" (kind of bunting, I think) were formerly found to contain large numbers

of halteridium only—no proteosoma, I am certain. They have often been used to refeed old infected stock. Yesterday they were examined again and found to contain numerous halteridium and swarms of proteosoma. One bird died last night. Liver enormous and black. (See photo VII.) Heart's blood contained an enormous young brood of proteosoma. The bird's blood practically consisted of parasites. There you have it full at last. This also proves that sparrows' proteosoma is communicable to "baias."

my birds
took pro.
and nothing
for
with
only given.
to future
more
be jimbly

night. Liver enormous
Heart's blood contained
enormous young brood of
The birds
consisted of
Then you
lost.
This also proves
proteosoma is communicable

Photo VII.*

Having won the battle of Atbara I am now marching on to Omdurman—that is, I am rushing the black-sausage-shaped bodies.

I wonder if the Secretary of State gave permission to publish results as soon as obtained. They ought to be published in the interest of the public. Thus, the Planters' Association of Southern India have written to me asking what I have done. I am obliged to tell them that I must not say. This is very absurd. It will be worse in the future. Events will march now and, instead of keeping things dark, every publicity should be given to such facts as those just found, as the communicability of malaria *via* the mosquito. At present my mouth is closed.

Well, I have become unbearable with conceit over tracking the

* Photograph of original letter of July 9th, 1898, in the possession of the London School of Hygiene and Tropical Medicine,

germinal rods into the salivary gland. That was a grand charge. I brag openly about it!

12th July.—I have just received yours of 24th and the B.M.J. of June 18th together. I am, indeed, sorry to hear of your illness, and, while it was very kind of you to have taken so much trouble with Lord Lister (and also in writing to me) wish that you had postponed doing so until better. I have felt inclined to telegraph and ask how you are. It was really very good of you to bother yourself, and I only hope you won't think of doing so again until quite well. We are partly selfish in wishing so, remember. However, at this distance it is scarcely possible to send wishes for even a return to health; and I can only trust that you are much better while I write. The scheme you are going to propose is admirable. No doubt the Government of India will jog up when Chamberlain comes along. Remember I am entirely at your disposal. The idea is grand, and I promise to throw myself into it. I have already arranged my tactics. They will be Napoleonic—concentration. We shall attack our problem at a time in a body, if possible. That is the way, I am sure. See the results I am getting out of *proteosoma* alone. If I had remained fiddling with *anchoyostomiasis* or human malaria, we should not have gone quarter the distance by this time. One thing at a time, and at it full speed. But I want to suggest my coming home for a few months—or rather you have suggested it—for the following reasons:—

1. At the rate I am now going I expect that I shall have solved the malaria problem in two months or three. I have only to fill in details of the germinal rod cycle, take photos, send you specimens, etc., and do a number of obvious experiments. At the same time I think I can carry out the "black body" business, grow them in water, or infect mosquito grubs with them. That will complete *proteosoma*. One single experiment with crescents (there are numerous dappled-winged mosquitoes here now) will enable me to bring malaria into line with *proteosoma*—they are sure to be just the same. This will really score all the chief points. I ought to do it in two months; it has taken me only one to work out the germinal rod cycle.

2. I should like to come home and take your advice about future work ; also to read up coccidia ; also, if possible, to go and see Labbé and Pfeiffer, etc. ; to read some papers before Societies ; to get some instruments I have thought of for counting eggs and gymno-sporidia, etc.

3. I have had $3\frac{1}{2}$ years' incessant work and worry ; am feeling tired and out of sorts, and feel I should have a rest and get a sniff of cold before opening into such a big business as a commission.

4. If the commission is to be started my presence in England may be useful to help you to arrange details.

Now I *may* apply for furlough as you suggested ; but I have just had to apply officially for six months' extension of my present duty and have not yet touched kala-azar. They are sure to reply "No ; you have applied for six months' extension ; you have not yet touched kala-azar : we can't give you furlough yet ; you must go to Assam."

I certainly would like and intend to run up to Assam (five days' journey from here) for a few weeks just to have a look at kala-azar ; but to deal with that disease thoroughly will take months, if I mistake not. Now there appears only one way out of this difficulty (unless I go sick), and that is, if the Colonial Office and India Office come to terms and have me sent home on duty. It will also save us money—and we are nearly ruined from having to live in hotels everywhere—family in one place, myself in another. If you think it advisable then, will you drop in a suggestion to that effect when they come to consider the details with you ? If, however, you *don't* think Government should be asked to send me home *on duty* and yet want me to come home, I shall certainly do so, but it will facilitate matters if you can send me a business note to the effect that the Colonial and India Offices have agreed to start a commission and that my presence in England would be useful. I could send this up to Government to back my application for furlough. I expect to be able to "rough hew" malaria and have a look at kala-azar by October, so as to start home in November, or even earlier. In fact, I may be perhaps free to go home very shortly—say, September.

Your book is out here at last—reviews appearing in the lay press

very favourable, but some reviewers still hanker over the "blue mist" theories—they save so much trouble. More of this anon.

Now I must thank you for the articles in the B.M.J. and all the handsome things said therein. It is useful having Laveran's Metchnikoff's and Nuttall's assent. The next thing in the B.M.J. ought to contain the full solution of the problem. I think the mistake about halteridium arose from Series IV (*vide* Report¹ and remarks about crows). I thought I corrected myself in later letters. The remarks at the end of the leader in the B.M.J. will probably stir Government to work things on a larger scale. But they may "wig" me for authorising you to publish at all. I shall reply to the point.

Two more of my artificially-infected sparrows have died from excessive proteosomiasis-swarms. I overdosed them. I hope to have a lot more birds (crows, sparrows) infected in time to wire you about 25th. Black bodies not yet found in healthy mosquitoes—probably proteosoma spores.

Remember, if there is a hurry about the commission, if you want men to come out quickly and learn the mosquito business, *I will stop out here as long as you think necessary*. But personally I feel I ought to go home for six months, unless you think the reasons urgent against it. Government won't hesitate over sending me home on duty and will think twice as much of the commission in consequence. Bully them, squeeze them, they deserve it, and will respect the whole business much more if you do. With kind regards, wishes for return of health and all felicitations of victory.

Yours sincerely,
(Signed) R. ROSS.

¹ Report on the cultivation of *Proteosoma*, Labbé, in Grey Mosquitoes, 'Ind. Med. Gaz., 1898,' XXXIII., No. 12, December.

